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John Buridan and Critical Realism

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Abstract
In this paper I examine what John Buridan has to say in his *Quaestiones in Analytica Posteriora* relevant to the subalternate mathematical sciences, particularly astronomy. Much previous work on the scholastic background to the Scientific Revolution relies on texts that were written in the late sixteenth or seventeenth centuries. Here I am interested in texts that might reflect the context of Copernicus, and, in particular those before 1500. John Buridan and Albert of Saxony were fourteenth century authors influential in Cracow in the fifteenth century, whose conception of science may be characterized as "critical realism." Their view would support the autonomy of astronomy, as well as the idea that sciences may progress over time.

Keywords
John Buridan, Albert of Saxony, Nicholas Copernicus, astronomy, subalternate sciences, *moderni*, critical realism, "*ideo quasi mendicare oportet intellectum humanum*."

Introduction

In this paper I describe the distinguishing characteristics of the image of subalternate mathematical sciences as found in the *Questions on the Two Books of Aristotle's Posterior Analytics* of John Buridan, and to a lesser extent in the *Very Subtle Questions on the Books*

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of the Posterior [*Analytics*] of Albert of Saxony. The main source for my discussion will be the transcription of Buridan's *Questions on the Two Books of Aristotle's Posterior Analytics* [hereafter *QuestPostAn*], made by Hubert Hubien from MS Liège, Bibliothèque de l'Université, cod. 44 C (= Grandjean 648), which is available electronically.[1] This work, however, has much in common with the *Questiones subtilissime Alberti de Saxonia super libros posteriorum* [hereafter *Questiones subtilissime*] published at Venice in 1497.[2] Earlier scholars have tried to untangle the *Quaestiones* of Buridan from those of Albert of Saxony, but much work remains to be done.[3] There are also manuscripts of two versions of an *Expositio* of the *Posterior Analytics* ascribed to Buridan. The 1985 dissertation of Bernd Michael still represents the *status quaestionis*.[4] I suggest that the nature of Albert of Saxony's *Questiones subtilissime* as published at Venice in 1497 could be explained if Albert, in his writing on the *Posterior Analytics*, began from Buridan's *QuestPostAn* rather than from the bare text of the *Posterior Analytics* itself. Albert's work is shorter than Buridan's and includes large subtle or sophistical digressions that might

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1) The transcription by Hubert Hubien, *Iohannis Buridani Quaestiones in duos libros Aristotelis Posteriorum Analyticorum*, was brought to my attention by the bibliography of Jack Zupko, *John Buridan. Portrait of a Fourteenth-Century Arts Master* (Notre Dame, 2003). It is available electronically on the website of Peter King: <<http://individual.utoronto.ca/pking/resources/buridan/QQ_in_Post_An.txt>> I thank Hans Thijssen for providing me with a link to the electronic text.

2) This was the only version of Buridan's or Albert's questions to receive an early printed edition. It has been reprinted (Hildesheim, 1986) with Albert's *Questiones subtilissime in libros Aristotelis de celo et mundo* (Venice, 1492).

3) See, for example, J.M.M.H. Thijssen, "Buridan, Albert of Saxony and Oresme, and a Fourteenth-century Collection of Quaestiones on the Physics and on De Generatione et Corruptione, *Vivarium* 24 (1986), 70-80.

4) Bernd Michael, *Johannes Buridan: Studien zu seinem Leben, seinen Werken und zur Rezeption seiner Theorien im Europa des späten Mittelalters*. Inaugural-Dissertation der Freien Universität Berlin (Berlin 1985). For works on the *Posterior Analytics* see items 11 (480-494; works of Buridan) and 30 (898-908; questions now ascribed to Albert of Saxony). Hubien's transcription omits what Michael lists, 487, as question 11 in the Buridan *Quaestiones* manuscript, no doubt because it appears out of place at the end of the work. I have consequently not been able to check the question, "utrum in omni demonstratione propter quid praemissae sint causae conclusionis et non in demonstratione quia."

be supposed to appeal to students, while following the overall structure of the *QuestPostAn* as found in the Liège manuscript. I will here give some samples from the *Questiones subtilissime* to suggest how it lives up to its title.

The research for this paper was done as part of a project sponsored by the Max Planck Institute for the History of Science in Berlin under the title "Before Copernicus." One of my tasks as a member of that project is to examine theories of science in the fifteenth century that might have been relevant to Copernicus. In a previous paper examining the *Quaestiones* on the *Posterior Analytics* of Walter Burley, I argued that Burley's conception of science kept the door open to scientific change and new knowledge derived from experience.[5] There is no evidence that I know of, however, that Burley's *Questions on the Posterior Analytics* were available in Cracow or in other centers where Copernicus worked (the questions are extant in only one manuscript, currently in Gonville and Caius College, Cambridge). In this paper, therefore, I have chosen to examine the same issues in the works of John Buridan and Albert of Saxony, because they *are* known to have been influential in the fifteenth century in Cracow.[6] In future work, I will examine other commentaries

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5) Edith Sylla, "The Status of Astronomy between Experience and Demonstration in the Commentaries on Aristotle's *Posterior Analytics* of Robert Grosseteste and Walter Burley," in *Erfahrung und Beweis: Die Wissenschaften von der Natur im 13. und 14. Jahrhundert*, ed. Alexander Fidora and Matthias Lutz-Bachmann (Berlin, 2007), 265–91.

6) See e.g. Paul W. Knoll, "The Arts Faculty at the University of Cracow at the End of the Fifteenth Century," in *The Copernican Achievement*, ed. Robert S. Westman (Berkeley, Los Angeles, London, 1975), 137-156, and, in the same volume, Nicholas H. Steneck, "Commentary in Defense of Context," 157-164. For logic in particular, see André Goddu, "The Logic of Copernicus's Arguments and His Education in Logic at Cracow," *Early Science and Medicine* 1 (1996), 28-68 at 38, "the following authors were often cited in logical works: Giles of Rome… Radulphus Anglicus (evidently Ralph Strode), John Buridan, Marsilius of Inghen (a follower of Buridan), Paul of Venice (an Averroist), Nicholas Orbellis and Peter Rosellus (both Scotists), and John Versoris (influenced by both Albert the Great and Thomas Aquinas). See also Mieczyslaw Markowski, "Dialektische und rhetorische Argumentation an der Krakauer Universität im 15. Jahrhundert," in *Argumentationstheorie. Scholastische Forschungen zu den logischen und semantischen Regeln korrekten Folgerns*, ed. Klaus Jacobi (Leiden,

on the *Posterior Analytics*, for instance those of Albertus Magnus, Thomas Aquinas, Marsilius of Inghen, Paul of Venice, John Versor, and of other scholastics whose views were known or influential before 1500.

To examine the ideas of Buridan and Albert of Saxony on the *Posterior Analytics* is all the more called for—beyond the fact that they may have been influential at the time of Copernicus—because previous work on the Aristotelian background to the Scientific Revolution has privileged versions of Aristotelianism that differ to a greater or lesser degree from the version represented by Buridan and Albert of Saxony.[7] It used to be assumed that "Renaissance Aristotelians" could be treated as a single group. As James Lennox so humorously put it, the label "Galileo's Aristotelian opponent" is "a uniformly colored blanket hiding a vast variety of philosophical and scientific shades and colors."[8] More recently, finer distinctions have been made between various flavors or strands of Aristotelianism, but the emphasis on later versions and on versions unlike that of Buridan and Albert of Saxony remains.

A good example of research that uses very late Aristotelian sources to represent what scholastic Aristotelians held is Zvi Biener's 2008 dissertation from the University of Pittsburgh, which attempts to show that the new science of Galileo and Descartes *more geometrico* draws on themes from the medieval Aristotelian conception of subalternate sciences. Biener argues that the Aristotelian position can be represented by the work of Eustachius a Sancto Paulo and Francesco

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1993), 577-587. In an email of 22 October 2008, André Goddu kindly summarized for me Markowski's more detailed findings as published in his articles in Polish: the views of the *moderni* were dominant at Cracow in the first half of the fifteenth century; the period 1461-1464 was dominated by Johannes Versor; the period 1464-1474 by Thomas Aquinas; the period 1475-1483 by what Markowski calls a 'historical Aristotelianism' with attention to Giles of Rome; 1484-1489 by Albert the Great; and 1490 to 1524 by doctrinal pluralism. Thus the situation at Cracow when Copernicus was a student was highly eclectic.

7) This would hold, for instance, for Jacopo Zabarella. See Heikki Mikkeli, "Giacomo Zabarella," in *Stanford Encyclopedia of Philosophy* <http://plato.stanford.edu/entries/zabarella> last accessed 8 October 2008.

8) James G. Lennox, "Aristotle, Galileo, and 'Mixed Sciences,'" in *Reinterpreting Galileo*, ed., William A. Wallace (Washington, D.C., 1986), 49.

Suarez, said to represent "the liberal Jesuit position."[9] Given Descartes' attendance at La Flèche, Jesuit Aristotelianism may indeed be the relevant stripe of Aristotelianism to compare to the ideas of Descartes, but the choice is less obvious in the case of Galileo.[10] One of the positions of Eustachius a Sancto Paolo that Biener takes to have had a possible influence on Descartes holds that all of the sciences are at least in a loose sense (*non omnino proprie*) subalternated to metaphysics.[11] Moreover, Biener quotes Descartes' statement in his *Principia* that, "The only principles which I accept, or require, in physics [*physica*] are those of geometry and abstract mathematics; these principles explain all natural phenomena" (*Principles* II §64).[12] If the foundations of science are *a priori* and if science is supposed to be certain, then this would quite naturally lead to Descartes' program to set aside what he had been taught in order to re-establish science on his own new and more secure *a priori* foundations.

Biener's purpose in his dissertation is to study the scholastic mixed-mathematical, middle, or subalternate sciences as pointing the way to the much broader early modern mathematization of science and to a revised classification of the sciences.[13] What I am suggesting here is that the picture Biener paints of the scholastic subalternate sciences as background to Descartes and Galileo should not, without

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9) Zvi Biener, "The Unity of Science in Early-Modern Philosophy: Subalternation, Metaphysics and the Geometrical Manner in Scholasticism, Galileo and Descartes," Ph.D. dissertation, University of Pittsburgh, 2008 < http://etd.library.pitt.edu/ETD/available/etd-03092008-215336/unrestricted/Biener_2008.pdf> retrieved 28 September 2008. Eustachius a Sancto Paolo's textbook, *Summa philosophiae quadripartita*, was first published in Paris in 1609 (Biener, 49). Francisco Suárez, *Disputationes metaphysicae* was published beginning in 1597 (Biener, 50). According to Biener, "liberal Jesuit scholasticism" (Stephen Menn's term) is "mostly devoid of dedicated Ockhamists, Averroists, Scotists and Thomists" (Biener, 51).

10) Cf. William A. Wallace, *Galileo's Logic of Discovery and Proof* (Boston Studies in the Philosophy of Science 137) (Dordrecht, 1992), as well as the many other publications of Wallace, and Rivka Feldhai, *Galileo and the Church. Political Inquisition or Critical Dialogue?* (Cambridge, 1995), ch. 10-12.

11) Biener, "The Unity of Science," 60 and 147.

12) Ibid., 9.

13) Ibid., 11.

further study, be assumed to apply to the background of Copernicus.

Buridan, Albert of Saxony, and the *moderni* as Critical Realists

How one might best describe the trends in the history of the theory of science, and particularly of the subalternate sciences such as astronomy, in one or another location during the fourteenth and fifteenth centuries depends to a great extent on the issues that one is most interested in, and a definitive picture is not currently available. For the theory of science available in Cracow in the fifteenth century, however, one alternative approach was that of John Buridan and those whose views were like his.[14] After considering other choices, I have decided, *faute de mieux*, to refer to Buridan, Albert of Saxony, Marsilius of Inghen, and others like them as *moderni* and to their way of approaching problems as the *via moderna*, although they might also have been labeled Ockhamist, nominalist, or terminist, depending upon the historian's sensitivities as to the meanings of these terms.[15] At the very least, Buridan and Albert were the sort of thinkers who, in the fifteenth century, came to be labeled *moderni* or followers of the *via moderna* even if this label was not attached to them by their contemporaries.[16] In the fourteenth century, the people later labeled *moderni*, including John Buridan and Albert of Saxony, but also William of Ockham and even Walter Burley, tended also to be Averroists, voluntarists, and

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14) Cf. Markowski, "Dialektische und rhetorische Argumentation."

15) Since Hans Thijssen begins his paper in this volume by saying "There was no such thing as the Buridan school," I have thought it best not to use the term, "the Buridan school."

16) See Maarten J.F.M. Hoenen, "*Via Antiqua* and *Via Moderna* in the Fifteenth Century: Doctrinal, Institutional, and Church Political Factors in the *Wegestreit*," in *The Medieval Heritage in Early Modern Metaphysics and Modal Theory, 1400–1700*, ed. Russell L. Friedman and Lauge O. Nielsen (Dordrecht, 2003), 9–36, and the works he cites by William J. Courtenay and others. See also Pepijn Rutten, "*Contra Occanicam discoliam modernorum*: The so-called *De universali reali* and the Dissemination of Albertist Polemics against the *via moderna*," *Bulletin de Philosophie Médiévale* 45 (2003), 132-165.

empiricists.[17] Whereas the *moderni* I am concerned with had their heyday in the fourteenth and early fifteenth centuries, there was another grouping of people, later labeled *antiqui*, who were more concerned with the certainty of science and therefore with *a priori* or rational foundations and with the unity of science (at least at first examination, the views Biener describes seem to be more like the *antiqui*). Although the central positions of the *antiqui* were laid down in the thirteenth century (hence the label *antiqui*), similar positions became popular again the sixteenth and seventeenth centuries. The *antiqui* included those who favored Avicenna over Averroes, and many Thomists such as Aegidius Romanus.[18] The reemergence of the *antiqui* position in the *Wegestreit* of the fifteenth and sixteenth centuries may have been connected, as far as commentaries on the *Posterior Analytics* are concerned, with new editions and translations of the Greek commentators on Aristotle and possibly with the new availability of Averroes's large commentary on the *Posterior Analytics*.[19]

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17) See Edith Sylla, "The A Posteriori Foundations of Natural Science; Some Medieval Commentaries on Aristotle's *Physics*, Book I, chapters 1 and 2," *Synthèse* 40 (1979), 147–87, and eadem, "Averroism and the Assertiveness of the Separate Sciences," in *Knowledge and the Sciences in Medieval Philosophy*. ed. Reijo Työrinoja, Anja Inkeri Lehtinen, and Dagfinn Føllesdal (Proceedings of the Eighth International Congress of Medieval Philosophy 3) (Helsinki, 1990), 71-80. Note that I say "tended," by which I mean only something like "more often than not."

18) In the commentary on the *Posterior Analytics* found among Galileo's early papers, one question ascribes alternate positions on the types of demonstration to Avicenna and to Averroes. See Galileo Galilei, *Tractatio de Praecognitionibus et Praecognitis and Tractatio de Demonstratione*, trans. William F. Edwards; intro. and notes William A. Wallace (Padua, 1988), 98–101. Avicenna is said to hold that there is only one species of demonstration, the *propter quid* demonstration that gives the cause of an effect. On the other hand, Averroes is supposed to hold that there are three species of demonstration: *propter quid*, *quia*, which proves existence but not cause, and a third type, the *potissima*, which proves both the cause and the existence of the effect. Galileo, or the author from whom he is excerpting, holds a third position, ascribed to Themistius, Philoponus, Algazali, and Thomas, saying that there are only two species of demonstration, the *propter quid*, and the *quia.*

19) Walter Roy Laird, "The *Scientiae Mediae* in Medieval Commentaries on Aristotle's *Posterior Analytics*" (Ph.D. dissertation, University of Toronto, 1983), ch. VIII, on Jacopo Zabarella, 218-222.

The tendencies of the *antiqui* were reinforced as a result of the decisions of the Council of Trent, which was first convened in 1545, two years after Copernicus's death. In an earlier paper, I argued that the climate created by the decisions of the Council of Trent helps to explain why Galileo was condemned in part for holding that the Copernican theory was "probable."[20] In that same climate, there was a tendency to consider the *moderni* to be "skeptics."[21]

Rather than skeptics, Buridan and the other *moderni* are better called, in Alan Musgrave's terms, "critical realists."[22] Musgrave's argument, against Pierre Duhem and those following him, was that the great astronomers of antiquity were not instrumentalists.[23] A key tool in his analysis was to distinguish "instrumentalism" from "critical

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20) Edith Sylla, "Galileo and Probable Arguments," in *Nature and Scientific Method*, ed. Daniel Dahlstrom, Studies in Philosophy and the History of Philosophy, vol. 22, (Washington, 1991), 211-234.

21) Although the intellectual strife of the time was mainly the result of theological controversies, the resulting passions spilled over to what seemed to be philosophical issues, such as nominalism versus realism. Indeed, during the fifteenth-century *Wegestreit*, as Hoenen describes it, each side accused the other of heresy, often presenting a distorted picture of what the other side held. Hoenen, *"Via Antiqua* and *Via Moderna*," 16, 22 ff. From the point of view of the *antiqui*, the *moderni* were tearing down the harmonious synthesis of knowledge put together by Thomas Aquinas and the *antiqui*. This has meant that some historians have looked primarily to the *antiqui* as potential carriers of the torch toward modern science (I do not have space here to determine whether Aquinas himself or the early modern Thomists should simply be considered *antiqui* in this matter or if a more complicated taxonomy is in order; the followers of Albertus Magnus are another group whose role must be taken into account). On the other hand, some historians of science have let Thomas Aquinas stand in for the scholastic opponents of the new science for the simple reason that English translations of Thomas Aquinas's work are more readily available than translations of the *moderni*. These and similar factors have contributed to the lack of attention to the *moderni* as potentially laying out a framework congenial to progress in mathematical astronomy. (I should note that, based on a reading of Hans Thijssen's paper in this volume, I should perhaps recognize that Pierre Duhem previously promoted the role of the *moderni* in the origins of modern science.)

22) Cf. Jack Zupko, "Buridan and Skepticism," *Journal of the History of Philosophy* 41 (1993), 191-221.

23) Alan Musgrave, "The Myth of Astronomical Instrumentalism," in *Beyond Reason: Essays on the Philosophy of Paul Feyerabend*, ed. Gonzalo Munévar (Boston Studies in the Philosophy of Science 132) (Dordrecht, 1991), 243-280. The article previously

realism." Whereas instrumentalists hold that "even a theory that is wholly correct does not describe anything but serves as an instrument for the prediction of facts that constitute its empirical content," critical realists may "concede the conjectural or hypothetical character of some scientific theory," but nevertheless hold that "a hypothesis is still a true or false description of reality, rather than a mere 'mathematical hypothesis.'"[24] The mistake of historians claiming that ancient and medieval astronomers were instrumentalists is to assume that the only alternative to instrumentalism is what Musgrave calls "dogmatic realism," holding not only that scientific theories are true or false descriptions of reality, but also that we can *know for sure* which theories are true and which are false. While aiming for theories that are true descriptions of reality, critical realists may admit in certain situations, for instance when there are competing or even conflicting viable theories, that science has not yet achieved certainty, while not giving up the search. A similar argument applies to Buridan, Albert of Saxony, and arguably to the *moderni* as a group—in questioning scientific dogmatism, they were not skeptics but critical realists.[25]

To Recommence the Inquiry into Principles

To see how a critical realist might operate in practice it is useful to look at Ibn al-Haytham. Near the beginning of his *Optics*, Ibn al-Haytham expresses clearly the attitude that a critical realist might take in the mathematical science of optics. He begins by explaining that previous scholars have been divided into two camps, with the more physical thinkers holding that rays come from objects into the eye (intromission theories of vision) and the more mathematical

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appeared in German in *Versuchungen Aufsätze zur Philosophie Paul Feyerabends* (Frankfurt, 1980).

24) Ibid., 243-244.

25) Cf. Amos Funkenstein, "The Dialectical Preparation for Scientific Revolutions," in *The Copernican Achievement*, ed. Robert S. Westman (Berkeley, 1975), 165-203, at 192, "The key methodological term in both Buridan's analysis and its revision by Oresme is *persuasio.* Both of them do not seek absolute demonstrations, but rather sufficient certitude."

thinkers holding that some sort of visual rays go out from the eye (extramission theories). When there are two conflicting theories like this, he says, there are several possibilities:

> Now, for any two different doctrines, it is either the case that one of them is true and the other false; or they are both false, the truth being other than either of them; or they both lead to one thing which is the truth. [In the latter case] each of the groups holding those two doctrines would have failed to complete its inquiry and, unable to reach the end, has stopped short of it. Alternatively, one of them may have reached the end but the other has stopped short of it, thus giving rise to the apparent difference between the two doctrines, although the end would have been the same had the investigation been pushed further. Disagreement may also arise in regard to the subject of any inquiry as a result of a difference in methods of research, but when the inquiry is rightly conducted and the investigation intensified, agreement will emerge and the difference will be settled.[26]

In view of the disagreement in optics, Ibn al-Haytham proposed to "recommence the inquiry into the principles and premises, beginning our investigation with an inspection of the things that exist and a survey of the conditions of visible objects."[27] From observations and induction, al-Haytham said he would ascend gradually and in orderly fashion. He admitted that he was "not free from that human turbidity which is in the nature of man," but says, "we must do our best with what we possess of human power."[28] In his *Reportata Parisiensia*, John Duns Scotus said that the subalternate sciences may obtain the evidence for their principles from experience, as had been proved by Ibn al-Haytham in his *Perspective*. And the trend I see in the ideas of Buridan may have roots in the work of Duns Scotus, who, among other things, may have transmitted to his European contemporaries Ibn al-Haytham's conception of sciences like optics combining mathematics and empiricism.[29]

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26) A.I. Sabra, trans. and intro., *The Optics of Ibn al-Haytham. Books I—III. On Direct Vision* (London, 1989), bk. I, ch. 1, sec. [5], 5. Sabra already drew attention to this passage as embodying a concept of experiment as a method of proof.

27) Ibid., sec. [6], 5.

28) Ibid., 6.

29) Alexander Fidora, "Subalternation und Erfahrung: Thomas, Heinrich und Duns Scotus," in *Erfahrung und Beweis. Die Wissenschaften von der Natur im 13. und 14.*

I propose that an early modern astronomer like Copernicus might have seen himself as following a path in astronomy like the path followed by Ibn al-Haytham in optics, recommencing the experiential inquiry into the principles and premises within an established field where there were conflicting theories. The image of mathematical sciences presented in Buridan's *QuestPostAn* and Albert of Saxony's *Questiones subtilissime* supports this conception of the role of an astronomer. As critical realists, the *moderni* endorsed an image of science that they found in Aristotle's *Posterior Analytics*. They were confident in the possibility of human knowledge of the natural world, and yet they did not dogmatically assert that current scientific theories were entirely certain. Instead of the certainty of science, they emphasized its empirical origins, and they treated each scientific discipline as separate, rather than supposing that all knowledge could be synthesized into a consistent whole under the umbrella of metaphysics or philosophy in general.

Subalternate Sciences and the "Prohibition of *Metabasis*"

In doing my research I have used the dissertations (and later articles) of Steven Livesey and Roy Laird.[30] Steven Livesey has made a special investigation into what he calls Aristotle's "prohibition of *metabasis*" or the attempt to combine material from disparate sciences within a single demonstration. He is particularly interested whether medieval

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Jahrhundert. Experience and Demonstration. The Sciences of Nature in the 13th and 14th Centuries, eds. Alexander Fidora and Matthias Lutz-Bachmann (Berlin, 2007), 195-206, at 205. In his "Induction according to Duns Scotus," in the same volume, 207-222, at 221 Steven Marrone expresses surprise at finding such an attitude in Scotus: "Was Duns then on the verge of proposing a natural science of probability or at least of a sceptical and almost early-modern less-than-absolute certitude? His words would surely point in that direction, though it is hard to believe that anyone in his day was truly prepared for so radical a departure from the epistemic standards of the still-regnant orthodoxy of an Aristotelianiazing theory of knowledge." My suggestion here is that it was only later that Aristotelianism became so orthodox, rather than empirical.

30) Steven Livesey, "*Metabasis*: The Interrelationship of the Sciences in Antiquity and the Middle Ages," (Ph.D. dissertation, University of California, Los Angeles, 1982); Laird, "The *Scientiae Mediae*."

thinkers overcame the prohibition of metabasis in order to use mathematical arguments within natural philosophy. While benefiting from the sources and analysis that Livesey provides, my interest here is a different one, because I am especially interested in attitudes toward astronomy, which was already a mathematical science in antiquity. Thus I am more interested in the status of astronomy as an autonomous mathematical science, and alert to a possible lack of independence of astronomy vis-à-vis natural philosophy.

In studying what Aquinas called the "middle sciences" between mathematics and natural philosophy, including astronomy, perspective (another name for optics), and harmonics, Roy Laird has studied which authors make the middle sciences subalternate to natural philosophy as well as to mathematics and which do not. According to Laird, whereas a few philosophers such as Aegidius Romanus and Paul of Venice suggested that perspective is subalternate both to geometry and to natural philosophy, Aristotle, and following him Robert Grosseteste, Thomas Aquinas, Walter Burley, and others supposed that perspective is subalternate only to geometry.[31] John Buridan and Albert of Saxony not only do not make perspective subalternate to natural philosophy, but they follow earlier thinkers in supposing that, if anything, natural philosophy, at least the relevant parts of it, may be under perspective. Thus experience of the rainbow includes facts that are explained by perspective. In like manner, one could treat the data of astronomy or the experience of astronomical phenomena as evidence leading to principles of mathematical astronomy from below or *a posteriori*.

Thus in Aristotle's terms, astronomy was said to be "subalternated" to geometry, and harmonics to arithmetic. In some cases, the higher subalternating science was said to be able to demonstrate *propter quid*, or on account of what, while the subalternated science could only demonstrate the same conclusion *quia*, or that.[32] This has sometimes been understood to mean something like Descartes' later claim

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31) Laird, "The *Scientiae Mediae*," 140 and 194: "That the intermediate sciences are subalternated to both mathematics and to *physica*—a notion first suggested by Aegidius …—turns out to be for Paul their distinguishing feature."

32) See ibid., 4-5; Alistair Crombie, *Robert Grosseteste and the Origins of Experimental Science, 1100-1700* (Oxford, 1953). Sometimes it is mistakenly thought that geometry

(see above) that the only principles he needed for physics were those of geometry and abstract mathematics, since mathematical principles could be used to prove physical conclusions. In fact, for the *moderni*, the vast majority of the principles of physics and even of astronomy come from experience and not from a higher science.

Geometry and *dignitates* in the Subalternate Sciences

This is the situation as envisioned by the *moderni*. If, in sciences like perspective and astronomy subalternated to geometry, geometry is to be used by the perspectivist or astronomer, it is only on the condition that geometrical principles or conclusions are "contracted" to match the subject matter of perspective. Thus where geometry speaks of lines, the geometrical theorems used in perspective are rephrased to speak of visual lines or rays.[33] The same outcome can be described by saying that the geometrical material appearing in perspective or astronomy does not appear explicitly at all. In Buridan's terms, the principles of any science include the subject, passions, and *dignitates*, which are principles for demonstrating passions of their subjects. Geometry enters perspective or astronomy not in the role of subject terms, but through the *dignitates* in the sense of warrants for inferences. In the syllogisms of the subalternate sciences such as optics and astronomy, all the explicit propositions are optical or astronomical, whereas the geometrical *dignitates*, when they are used, are most commonly tacit inference rules.

Many scholastic authors make this clear. In his commentary on the *Posterior Analytics*, bk. I, tract. III, ch. 3, Albertus Magnus wrote that *dignitates* seldom appear explicitly in ostensive demonstrations, or, if they do appear, they are "contracted" to the subject of the special science concerned.[34] He went on to explain how this would

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can prove *proper quid* the conclusions of optics, when in fact geometry can only prove these propositions "in their universal," not as optical.

33) See Sylla, "Status of Astronomy," 278-282.

34) Albertus Magnus, *Posteriorum Analyticorum Liber Primus* in *Opera Omnia*, vol. 2, ed. August Borgnet (Paris, 1890), 74, 75: "Est autem quoddam principium commune quod est de quodlibet affirmare vel negare, quod aliquando demonstrationem

play out in geometry and in special sciences subalternated to geometry.[35] Likewise, in his *Summa Logicae*, part III, ch. 4, William of Ockham states clearly that of the propositions required for demonstration, some are not explicitly part of the demonstration. Among the propositions that do not appear explicitly in the demonstration are such laws of logic as the law of non-contradiction. In the mathematical sciences including astronomy as well as arithmetic or geometry, a proposition such as "when equals are taken from equals, equals remain" would be presupposed without appearing explicitly, unless with the terms "contracted" to fit within the subject matter of the subalternate science.[36] Geometry might have a demonstration *propter quid* of a proposition analogous to a proposition in optics, but it would concern only triangles or other geometrical figures and not light rays: it would not prove *propter quid* an optical conclusion. On the other hand, a perspectivist might demonstrate a conclusion *propter quid* where, in his demonstration, the stated premises and conclusion would be optical, but the unstated "maxim" by which the conclusion followed from the premises would be geometrical, rather than simply logical.

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ostensivam ingreditur, sed raro…. Hoc autem principium quod commune est valde, quod dicit quod omne quod est vel non est, est affirmare vel negare, requiritur in demonstratione quae est ad impossibile; quia illa per illud confirmatur. Hoc autem non semper accipit quaelibet scientia particularis demonstrativa secundum suam communitatem universaliter, sed contractum ad suum genus in quantum illi generi sufficiens est…."

35) Ibid., 77-79.

36) William of Ockham, *Summa Logicae,* = *Opera Philosophica*, vol. I (St. Bonaventure, 1974), 509-510: "Est autem primo sciendum quod propositionum requisitarum ad demonstrationem quaedam sunt partes demonstrationis, sicut duae praemissae et una conclusio, et quaedam non sunt partes demonstrationis. Et vocantur dignitates et maximae vel suppositiones, quae sub propria forma non ingrediuntur demonstrationem, virtute tamen illarum propositionum aliquo modo scientur praemissae demonstrationis…. Propositio autem requisita ad demonstrationem, non tamquam pars, subdividitur, quia quaedam est talis quod necesse est quemlibet docendum habere eam, cuiusmodi sunt tales 'quidlibet est vel non est', 'de quolibet affirmatio vel negatio' et huiusmodi. Quaedam sunt tales quas non est necesse quemlibet docendum habere, sed necesse est aliquos artifices speciales eas habere, sicut est de istis 'aliquid est mobile', 'si ab aequalibus aequalia demas' etc."

In his *Summulae de dialectica*, Buridan talks about the principles of a discipline that ground the consequences rather than appearing as premises:

> But I say that in the demonstrations of these conclusions, not only are two first principles required, namely, the two premises, but also several others, for a demonstration requires not only the evidentness of the premises but also the evidentness of the consequence. But that consequence is a proposition, albeit a hypothetical one. And so, if the consequence is evident in itself, then it is an indemonstrable principle….[37]

Here the warrants for the inferences that others called *dignitates* Buridan labels "consequences." Thus what happens in the case of the subalternated sciences like astronomy is that the rules of reasoning in mathematics are taken to be as certain as the rules of reasoning in logic. They allow consequences (i.e. inferences) to be drawn. With all this in mind, it is time to look at the texts of Buridan's and Albert of Saxony's questions on the *Posterior Analytics* for support of the characterizations of the position of the *moderni* that I have given.

Questions and Answers of Buridan and Albert of Saxony on the *Posterior Analytics*

Three questions from Buridan's *QuestPostAn* and the related questions in Albert of Saxony's *Questiones subtilissime* will be used to characterize the aspects of their conception of science relevant here: 1) that science or human knowledge of the world is possible; 2) that sciences like astronomy use mathematics together with principles based on experience; and 3) that, although natural scientists should have confidence in their principles, the existing state of knowledge is not perfect and at some point an improved set of principles might be called for.

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[37] John Buridan, *Summulae de dialectica*, trans. Gyula Klima (New Haven, 2001), 714.

Book I, Q. 2. Whether it is Possible for us to Know Something.[38]

Buridan's conclusion is that just as fire is naturally inclined to heat, and heavy bodies to descend, so is the human intellect naturally inclined to understand objects sufficiently presented to it and also naturally inclined to comprehend the truth of first complex principles.[39] For science certitude of truth, certitude of assent, and evidence (or evidentness) are required. The greatest evidentness belongs to propositions, such as logical first principles, to which it is impossible to dissent. But there is also natural evidentness. In the natural case, we can be deceived by supernatural causes, but not in the common course of nature. Natural evidentness is sufficient in natural science. Although experience that is scanty and not carefully examined may deceive, when there is a great deal of experience examined in diverse cases, experience does not deceive. Although one cannot give a formal proof that the universal proposition one arrives at is true, one accepts the proposition as a known and naturally evident universal principle.[40]

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38) Compare Albert of Saxony, *Questiones subtilissime*, qq. 3-4, ff. 3vb-5va: "Utrum, quia aliqui antiqui dixerunt quod impossibile est nos aliquid scire, sicut achademici; alii autem dixerunt quod bene possibile est nos aliquid scire, sed impossibile est nos scire aliquid de novo, sicut Platonici; ergo primo videndum est an possibile sit nos aliquid scire; secundo an possibile sit nos aliquid scire de novo."

39) Buridan, *QuestPostAn*: "Unde debetis notare quod cum ponantur tres operationes intellectus, in earum qualibet sunt aliqua per se manifesta ex natura et inclinatione ipsius intellectus ad veritatem. Unde sicut ignis est naturaliter inclinatus ad calefaciendum et grave ad descendendum, ita intellectus est naturaliter inclinatus ad intelligendum obiecta sibi sufficienter praesentata, et etiam naturaliter inclinatus ad comprehensionem veritatis primorum principiorum complexorum; ideo non oportet quod per aliud primum iudicium declaretur…."

40) Ibid.: "Sed de evidentia debetis notare quod 'evidentia' multipliciter accipitur. Uno modo proprissime, et tunc evidentia propositionis dicitur secundum quam intellectus per suam naturam cogitur propositioni assentire et non potest ei dissentire; et isto modo diceret Aristoteles quod primum principium est nobis evidens. Secundo modo 'evidentia' dicitur quia cuilibet apparet et per nullam rationem humanam nisi sophysticam [*corr. ex* subiectivam ref. Albert of Saxony, *Questiones subtilissime*, f. 4rb] posset oppositum apparere; et isto modo sunt evidentia principia naturalia et conclusiones naturales. Et notandum quod haec evidentia non dicitur proprie 'evidentia' : quia circa tales propositiones evidentes intellectus posset decipi per causam supernaturalem; quia deus posset facere ignem sine caliditate, et posset facere in sensu meo et conservare

In his *Physics*, Buridan essentially makes accepting the truth of principles derived from repeated sense, memory, and experience a norm of the natural philosopher or student of ethics. He says that "We should concede that there cannot naturally be a vacuum as known by that means which is sufficient for positing and conceding principles in natural science," and "One who does not want to concede such declarations in natural and moral science is not worthy of having a large part in them."[41]

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speciem sensitivam sine obiecto, et ita per istam evidentiam tu iudicares ac si obiectum esse praesens, et iudicares falsum. Tamen illa evidentia naturalis bene dicitur naturalis, quia secundum illam non potest homo decipi stante communi cursu naturae, licet deciperetur per causam supernaturalem; et haec evidentia sufficit ad naturalem scientiam…. Istis visis, facile est solvere rationes…. 7. Ad aliam rationem… saepe per rationem intellectus evidenter corrigit errorem sensus…. 9. Ad aliam dicentem quod experientia saepe fallit, dicitur primo quod multa sunt principia evidentia non credita per experientiam, sed per manifestam terminorum inclusionem ad invicem, vel exclusionem, scito quid nominis. Sic enim est evidens quod homo est animal et asinus est animal. Deinde principia sumpta per experientiam adhuc habent naturalem evidentiam. Quia licet experientia pauca et parum examinata saepe fallat, tamen experientia multa et bene in diversis casibus examinata numquam fallit. Et quando ultra opinatur quod experientia numquam gratia formae concludit universale principium, quia numquam fit in omnibus singularibus, respondet Commentator, secundo *Physicorum*, quod licet inductio, sive experientia inductiva, non concludat gratia formae, tamen intellectus, ex eius naturali inclinatione ad veritatem, percipiens multotiens ita fieri quod non potest nec potuit recipere instantiam, nec videre esse rationem quare in aliis debeat esse aliter, ipse concedit universale principium tamquam notum et evidens evidentia naturali et possibili circa tali."

41) Buridan, *Sutilissime questiones super octo phisicorum libros Aristotelis* IV, 7 (Paris 1509; reprint Frankfurt, 1964), f. 73v, reply to the question, "Utrum possibile est vacuum esse": "Item omnis propositio universalis in scientia naturali debet concedi tanquam principium quae potest probari per experimentalem inductionem sic quod in pluris singularibus ipsius manifeste inveniatur ita esse et in nullo nunquam apparet instantia. Sicut enim bene dicit Aristoteles quod oportet multa principia esse accepta et scita sensu memoria et experientia. Immo aliquando non potuimus scire quod omnis ignis est calidus, sed per talem inductionem experimentalem apparet nobis quod nullus locus est vacuus… . Ideo debemus concedere quod non potest naturaliter esse vacuum tanquam scitum per illum modum qui est sufficiens ad ponendum et concedendum principia in scientia naturali." Also *Super octo physicorum libros* I, 15, ff. 18vb-19ra: "Secunda conclusio est quod necesse est omne quod fit naturaliter fieri ex subiecto presupposito vel in subiecto presupposito. Forma enim non fit integraliter ex subiecto presupposito, sed in eo. Sed tamen non puto quod hec conclusio sit

It should be understood that this is not a "skeptical" position, but one that recommends to natural scientists a justifiable degree of confidence in their results.[42] Albert of Saxony gives essentially the same answer to this question, but he spices it up with typical subtle or sophistic condiments. The point that the normal course of nature and our confidence in the veracity of our experience may be broken into by God's absolute power is phrased to allow the scholastic favorite "you are an ass":

> Whether it is possible for us to know something. It is argued that it is not possible… for it is possible that by some divine power there is produced in me a species representing heat and that heat is destroyed and cold induced and the action of the cold on sense is suspended…. By this argument it could be proved that it is not evident to me that you are a man, but that I may rationally doubt whether you are an ass or a goat. For the visible species which is in the organ of my visual power representing to me that you are a man could be conserved in my soul while you were transubstantiated into an asinine substance by the power of the First Cause.[43]

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demonstrabilis. Sed est declarabilis per inductionem in qua non inventa est instantia. Sic probat enim eam Aristoteles et tale reputari debet principium in scientia naturali. Aliter enim tu non posses probare quod omnis ignis est calidus, quod omne reubarbarum est purgativum colore, quod omnis magnes vel adamas est attractivus ferri. Et tales inductiones non sunt demonstrationes, quia non concludunt gratia forme, cum non sit possibile inducere in omnibus suppositis, sicut dicitur secundo *Posteriorum* quod multa principia indemonstrabilia fiunt nobis manifesta sensu, memoria, vel experientia. Experientia ex multis significationibus et memoriis deducta non est aliud quam inductio in multis singularibus per quam intellectus, non videns instantiam nec rationem instandi, cogitur ex eius naturali inclinatione ad veritatem concedere propositionem universalem. Et qui non vult tales declarationes concedere in scientia naturali et morali non est dignus habere in eis magnam partem."

42) Thus I think that previous historians who have labeled Buridan a skeptic are mistaken. Cf. Peter King, "Jean Buridan's Philosophy of Science," *Studies in the History and Philosophy of Science* 18 (1987), 109-132, at 109. Although King states that Buridan "attempts to chart a course through these skeptical waters," he says that, "what Buridan gives up in the final analysis is nothing less than the truth itself" (110).

43) Albert of Saxony, *Questiones subtilissime*, f. 3vb: "An possibile sit nos aliquid scire et arguitur quod non, quia non possumus de aliquo habere evidentiam sine formidine ad oppositum. Ergo non est possibile nos aliquid scire. Consequentia tenet, ex eo quod scientia est evidentia sine formidine ad oppositum. … et antecedens probatur, quia non possumus de aliquo habere notitiam nec evidentiam per sensum nec

Of course, Albert's reply to this argument is that although we cannot have the highest evidentness from sense, nevertheless we can have natural evidentness, which is sufficient for us to know.[44] But then Albert goes on to another scholastic favorite, the division of time into small parts. It is not evident, he says, that you were a man for a year, although it appeared to me for a year that you were a man. Because it is possible that you were only a man for half a year. The First Cause might have created you for an insensible part of a year, then annihilated you for another insensible part of the year, then recreated you, and so forth up to the end of the year. For the whole year you would have appeared to me as a man, but you would only have been a man for half a year.[45] This argument is already part of the reply to the principal arguments and so receives

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per intellectum, ergo nullo modo…. non per sensum. Nam quantumcumque appareat mihi quod ignis per sensum sit calidus, tamen propter hoc non habeo iudicium et evidentiam de hoc quod ignis sit calidus sine formidine. Nam possibile est quod per aliquam potentiam puta divinam producatur in sensu meo species representativa caliditatis, et quod caliditas destruatur et frigiditas inducatur et actio frigiditatis in sensum suspendatur. Isto posito apparet mihi quod ignis esset calidus et in veritate esset frigidus. Et cum casus positus sit possibilis, cum Prima Causa sit agens liberum, sequitur quod quotienscumque video ignem habeo dubitare an caliditas ignis sit corrupta et frigiditas inducta et species caliditatis in sensu sit representata et actio frigiditatis in sensu sit suspensa per Primam Causam…. Per istam rationem probaretur quod non est mihi evidens te esse hominem, sed quod rationabiliter habeam dubitare te esse asinum vel capram. Nam species visibilis que est in organo meo virtutis visive representans mihi te esse hominem posset in animo meo conservari te transubstantiato in substantiam asini per potentiam Primae Causae. Et quia hoc Prima Causa potest facere quando vult, sequitur quod quandocunque apparet mihi quod tu sis [corr. ex. scit] homo, habeo dubitare utrum sis asinus."

44) Ibid., f. 4va: "Ad rationes. Ad primam dico quod bene probatur quod mediante sensu non possumus habere de aliquo evidentiam summam. Nihilominus mediante ipso possumus habere evidentiam naturalem que sufficit nobis ad hoc quod sciamus."

45) Ibid., f. 4va, "Unde evidentia summa non est mihi evidens quod tu per unum annum fuisti homo quamvis per totum annum apparuit mihi quod tu esses homo. Unde possibile est quod tu per integrum annum continue apparuisti mihi homo. Et tamen non nisi per medium annum fuisti homo. Si enim Prima Causa per unam partem insensibilem anni te crearet, et iterum per aliam partem insensibilem te annihilaret, et iterum per sequentem insensibilem te recrearet, et sic de aliis usque ad finem anni. In fine anni verum esset dicere: per integrum annum mihi apparuisti homo, et tamen non nisi per medietatem anni fuisti homo, ex eo quod solum per tempus insensibile

no reply. From Albert's jocularity it should not be inferred, however, that he was not serious about the subject matter at hand, nor that he was skeptical, but only that he wanted to keep the students awake and paying attention. Just before he began to reply to the principal arguments he had continued to talk of asses while stating his clear position:

> For the solution of certain arguments it should be noted that something is said to be evident in two ways. There is the highest evidence [or evidentness] and there is natural evidentness. The highest evidentness of a proposition is when the intellect by its nature is compelled to assent to the proposition and cannot dissent from the proposition. In this way of speaking of evidentness we say that the principle "of anything, it is etc. [i.e. or it is not]" is evident. And speaking similarly of evidentness it is evident to me that I exist. But natural evidentness is when something is evident to us in the sense that by no human argument unless a sophistic one can the opposite be made plausible. And in this way natural principles and natural conclusions are said to be evident. And note that this is not evidentness in the strictest sense, because the intellect concerning things that are evident in this way can be deceived by supernatural power. Thus, as it was argued, a supernatural cause could conserve the species and similitude of a man in an eye, for example Socrates in my eye, and transmute that Socrates into an ass, and then Socrates would appear to me as a man but nevertheless he would be an ass. Therefore speaking of the highest evidentness it is not evident to me that you are a man, nor similarly is it evident to me that fire is hot. And those for whom this natural evidentness would not suffice would not be skillful demonstrators.[46]

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non fuisti et iterum per tempus insensibile fuisti et iterum non fuisti per tempus insensibile…."

46) Ibid., ff. 4rb-va, "Pro solutione aliquarum rationum notandum quod dupliciter dicitur esse evidentia. Quedam dicitur evidentia summa, quedam naturalis. Evidentia summa alicuius propositionis est secundum quam intellectus per suam naturam cogitur assentire illi proposioni et non potest dissentire illi propositioni. Isto modo loquendo de evidentia dicimus istud principium: de quolibet esse etc. esse evidens. Et similiter loquendo sic de evidentia evidens est mihi quod ego sum. Evidentia autem naturalis est quando aliquid est nobis sic evidens quod per nullam rationem humanam nisi sophysticam oppositum posset apparere. Et isto modo principia naturalia et conclusiones naturales dicuntur esse evidentes. Et nota quod hec non est evidentia proprissime dicta, quia intellectus circa illa que taliter sunt evidentia posset decipi per potentiam supernaturalem. Unde sicut arguebatur: causa supernaturalis posset speciem et similitudinem hominis in oculo puta Sortes in oculo meo conservare, et illum Sortem in asinum transmutare, tunc Sortes appareret mihi homo et tamen esset asinus. Ergo loquendo de evidentia summa non est mihi evidens quod tu es homo, et similiter

This last sentence mirrors the similar sentences quoted from Buridan earlier, indicating that the natural philosopher ought to have confidence in the principles and conclusions of natural philosophy. Indeed, the person who does not accept principles based on carefully examined experience is not worthy of having a large part in natural or moral science.

Q. 23. Whether it is Possible for a Demonstrator to Descend from One Genus to Another.[47]

Buridan admits that it is difficult to say whether it is possible for a demonstrator to descend from genus to genus because the question has hardly been discussed and it involves the issue of the distinction of the sciences. Sciences have unity because of the unity of their subjects, but the subject of one science can contain within itself the subject of another science contracted in some way, as the subjects of perspective and astronomy contain within themselves geometrical magnitude contracted to the specific subject area. The same *dignitates* can appear in diverse sciences, contracted to each science, as the axiom "if you subtract equals from equals, equals remain," becomes in geometry, "if you subtract equal magnitudes from equal magnitudes, equal magnitudes remain," and in arithmetic "if you subtract equal numbers from equal numbers, equal numbers remain." When the conclusions of a subalternating science are accepted as principles in a subalternated science, they are accepted not in their generality, which exceeds the bounds of the subalternated science, but as "contracted" to fit the subalternate science.[48] This not infrequently happens:

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est mihi evidens quod ignis est calidus, et quibus non sufficeret illa evidentia naturalis non essent habiles ad probandum."

47) Cf. Albert of Saxony, *Quaestiones subtilissime*, q. 24, "Utrum possibile sit demonstrantem descendere de genere in genus," ff. 18vb-19rb.

48) Buridan, *QuestPostAn*: "Ista quaestio, ut mihi videtur, est difficilis, primo quia valde modicum discussa est inter philosophos et doctores, secundo quia tangit ad modum distinctionis scientiarum, et est multum difficile assignare unde et quo modo scientiae accipiant originaliter suam distinctionem.… Deinde, dico, quantum ad principia, quod numquam sic descendetur de una scientia in aliam quod principium vel conclusio unius fiat principium alterius nisi aliquo modo diversificetur per contractionem,

> Now it is certain that natural science often supposes from mathematics, as in the consideration of the rainbow and of the proportions of motions.[49]

Albert of Saxony's answer to the parallel question is similar and without sophistical decoration.

Q. 25. Whether the Mathematical Sciences are the Most Certain of the Other Sciences.[50]

Buridan concludes that mathematical sciences are quite uncertain (*multum incerta*), but that the means of demonstration in mathematical sciences is most certain. So there is a conditional certainty in mathematics: if the principles are true, then the conclusions are true. So, for example, geometry is true if continua are not composed of indivisibles, which is a geometrical principle, not to be determined or demonstrated by the geometer himself.[51]

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quoniam scientia communior non utitur principiis propriis scientiae specialis, et etiam scientia specialis non utitur principiis vel conclusionibus scientiae superioris in tota eorum communitate, sicut apparet, sed cum contractionibus, ut dictum fuit. Tamen, ultimo, dico quod si per 'descensum de una scientia in aliam' intelliges quod principia vel conclusiones unius scientiae fiant cum quadam contractione principia alterius scientiae, tunc valde multipliciter contingit descendere de una scientia in aliam. Quoniam quandocumque supponit aliqua ab alia, contingit dicto modo descendere de illa a qua supponitur in illam quae supponit."

49) Buridan, *QuestPostAn*: "Modo certum est quod scientia naturalis saepe supponit a mathematica, ut in consideratione de iride et de proportionibus motuum."

50) Cf. Albert of Saxony, *Quaestiones subtilissime*, q. 26, "Utrum scientie mathematice sint certissime," ff. 19vb-20rb.

51) Buridan, *QuestPostAn*: "Credo etiam quod isto modo mathematica scientia est multum incerta. Quia principia mathematicalia indigent bene explanatione per superiores scientias; et etiam multae conclusiones mathematicae indigent expositione eius quod est dubium et a multis non concessum, <ut> scilicet quod linea non sit composita ex punctis; verbi gratia, conclusio in geometria est <quod possibile est> omnem lineam per aequalia dividere, quod esset impossibile de linea punctorum imparum, et etiam ita ista conclusio esset falsa quod diameter est incommensurabilis costae, et sic de multis aliis; tamen geometria non habet determinare nec demonstrare illam dubitationem. Ultimo <modo> certitudo attenditur ex certo modo demonstrandi suppositis principiis. Et tunc esset concedendum quod mathematicae demonstrationes essent in primo gradu certitudinis: quia demonstrationes mathematicae maxime observant

Albert reaches the same answer, that the inferences of mathematics are the most certain, but the principles are not.[52] He uses the occasion of the question to give the students some mathematical practice. For instance, applying the maxim "if equals are subtracted from equals, equals remain" to proportions (ratios in modern terminology), he first gives examples that misapply the maxim to ratios, before explaining the proper sense in which the maxim is true.[53] In other principal arguments, like Buridan, he raises the problem of subtracting or adding an indivisible to a finite quantity.

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formam syllogisticam et modum et maxime sunt ex propriis mediis. Et si est dubitatio in huiusmodi demonstrationibus mathematicis, est maxima certitudo ex suppositione principiorum, et est tamquam certitudo condicionalis quod si ita est sicut <principia ponunt, ita est sicut> conclusio ponit."

52) Albert of Saxony, *Questiones subtilissime*, f. 20ra-b: "Tertio modo certitudo scientie attenditur penes hoc quod principia illius scientie sunt notissima et evidentissima non ab alia presupposita et probata. Quarto modo certitudo scientie attenditur penes hoc quod illa scientia presuppositis suis principiis et concessis, firmissime et evidentissime concludit et demonstrat conclusionem ex illis principiis…. Tertia conclusio: loquendo de certitudine tertio modo dicta scientia metaphysica omnium aliarum scientiarum est certissima. Patet quia eius principia sunt certissima, nec presupponit ea ab alia scientia, sicut tamen alie scientie faciunt. Igitur etc. Quarta conclusio: loquendo de certitudine quarto modo scientia mathematica aliis scientiis est certior. Probatur quia scientie mathematice certiorem habent modum demonstrandi suas conclusiones et magis evidenter. Unde principiis mathematice presuppositis et concessis necesse est conclusiones ex illis principiis demonstratas concedere. Et ille consequentie quibus conclusiones mathematice inferuntur ex suis principiis sunt sic certe et evidentes: quod statim concessis principiis oportet conclusionem concedere. Et propter talem certitudinem et evidentiam consequentie conclusionis ex premissis scientie mathematice reputantur certiores aliis scientiis. Et hoc intendebat Aristoteles in littera."

53) Ibid., f. 19vb. He takes the equal ratios 8 to 4 and 6 to 3. He first proposes subtracting 2 from the first term of each of these ratios. When that is rejected, he proposes subtracting 1 from all four terms. Neither of these procedures results in equal ratios. In reply to the principal arguments, f. 20rb, he proposes subtracting "half a double ratio" from each of the ratios, and subtracting a "third part of a double ratio" from each, and finds in each case that the remaining ratios are equal, in the first case "half a double ratio" remaining for each.

Natural Evidentness and the Possibility of Scientific Change

If I am right that Buridan and Albert of Saxony were critical realists, then it is also relevant to consider what action they might have proposed if, in the course of working *within* an already established scientific discipline, the scientist began to sense flaws in the structure, and especially in the principles of the science. Did their image of science, and in particular of a subalternate mathematical science like astronomy, provide for the possibility of scientific change, as I claimed in my earlier article was the case for Walter Burley?

It is widely agreed that Greek and medieval astronomy set itself the problem of saving the phenomena by use of uniform circular motion. This is sometimes called the Platonic axiom, but in Aristotelian astronomy the principle of uniform circular motion of the star sphere would surely be based on experience, just like the principle that fire is hot. On the other hand, the principles that the sun, moon, and other planets move with combinations of uniform circular motions would be an extrapolation from the principle concerning the star sphere, but one that had been shown justified by the successes of Eudoxus, Hipparchus, Apollonius, and the other early astronomers. Thus, I claim that the so-called Platonic axiom was never taken as dogmatically as is often implied.

In the *Almagest*, Ptolemy simply introduced the principle of uniform circular motion of the heavens without justification. For instance, in Book III, ch. 3, on the hypotheses for uniform circular motion, he wrote:

> Our next task is to demonstrate the apparent anomaly of the sun. But first we must make the general point that the rearward displacements of the planets with respect to the heavens are, in every case, just like the motion of the universe in advance, by nature uniform and circular.[54]

In the fifteenth century, Albert of Brudzewo, professor of astronomy at the University of Cracow at about the time when Copernicus

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[54] Ptolemy, *The Almagest*, trans. and annotated by G.J. Toomer (New York, 1984), 141.

was a student, wrote in his commentary on Georg Peurbach's *Theorica nova planetarum*:

> Even though it is a first principle of astronomy that the sun moves regularly in its eccentric (and therefore one should not dispute in astronomy with anyone who denies this principle), nevertheless this principle can be demonstrated by the subalternating science, that is mathematics, as follows: the sun describes equal angles in equal times around its center [of motion] and cuts off equal arcs, therefore it is moved uniformly…. [55]

How does mathematics demonstrate the principle that heavenly bodies move in uniform circular motion? If Brudzewo's statement is examined carefully, mathematics enters his demonstration only on the assumption that the positions of the sun in relation to the center at different times are known. Then geometry is used to show that such positions are consistent with a uniform circular motion. This is crucial to understanding what is going on: mathematics is used as a way of inferring from observed positions of the sun at stated times that the sun is undergoing uniform circular rotation (and, since this is the case, that its circular rotation is eccentric to our point of observation). Of course, the difficulty here is that the inference from the measured positions to the theory is not unique—if there is a valid epicycle model, there will also be a valid eccentric model, and vice versa. Nevertheless. the predicted positions, as far as observation is concerned, will be the same.

In the first book of the *Physics* Aristotle had argued that a geometer need not debate those who deny his principles and likewise a physicist need not debate those who deny that things move, since this is a first principle of physics.[56] If, as was the case, from Greek antiquity it was a principle of mathematical astronomy that the

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55) Albertus de Brudzewo, *Super theoricas novas planetarum*, ed. Ludovicus Antonius Birkenmajer (Cracow, 1900), 30-31: "Etsi Solem in suo ecentrico regulariter moveri sit primum principium in astronomia (ideo cum negante illud, non est amplius in Astronomia disputandum), tamen tale principium potest per scientiam subalternantem, scilicet mathematice demonstrari sic. Sol in temporibus aequalibus aequales super centrum suum describit angulos et aequales resecat arcus, ergo aequaliter movetur…."

56) Sylla, "The A Posteriori Foundations of Natural Science."

heavenly bodies move in uniform circular motion, under what circumstances might an astronomer dispute concerning it?

As a critical realist in Alan Musgrave's terms, John Buridan believed that the principles of any genuine science should be certain, but may in fact be violated if for no other reason than that God, by his absolute power, can break the laws of nature. If long experience leads to a conclusion, no cases are known in which it does not hold, and no arguments against it have been proposed except sophistical ones, then a natural philosopher ought to assert it as true or else give up any claim to be a natural philosopher.[57] On the other hand, if there was evidence against a principle, or arguments that were not sophistical, might a scientist indeed adopt a new or changed principle?

In his questions on the *Physics*, Buridan had faced the problem of what to do, not when experience contradicted a physical theory, but when the injunction of the theologians that the philosopher should not deny that God can do anything that is not a logical contradiction forced the natural philosopher to ask what would happen if God broke into the normal course of nature, for instance, by annihilating everything inside the sphere of the moon.[58] Would there be a vacuum where the earth and its atmosphere had previously been, or would there be nothing, not even empty space, that is, no measurable extension? Seeing that natural philosophy did not provide an unambiguous answer to such a question, and that one was supposed simply to believe it (*hoc simpliciter credendum est*), Buridan concluded that it is necessary for the intellect so-to-speak to beg (*ideo quasi mendicare oportet intellectum humanum*). Having mistranslated this phrase in an earlier article, I now suggest taking the correct translation as a pointer to what the astronomer or natural philosopher might do when his principles turn out to be

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57) See texts in note 41.

58) Edith Sylla, "'*Ideo quasi mendicare oportet intellectum humanum*': The Role of Theology in John Buridan's Natural Philosophy," in *The Metaphysics and Natural Philosophy of John Buridan*, ed. J.M.M.H. Thijssen and J. Zupko (Leiden, 2001), 221-45. In this paper I mistranslated the Latin phrase in the title to say that one must "beg the intellect" rather than that the intellect must beg.

inadequate or faulty in some respect.[59] It is, after all, the intellect with which we know principles, whereas we have scientific knowledge of demonstrated conclusions. What would it mean for the intellect to beg?

In the question of his *Physics* where Buridan made this comment, he had been discussing what could be said about the duration of three angels created before the creation of the rest of the cosmos, if first God created one alone and then two more together. Then it seemed that there was nothing by which duration could be measured unless by the angels themselves or by God, but none of them are extended in a temporal dimension. Having commented that the human intellect must so-to-speak beg, Buridan went on:

> On this it seems to me plausible (*probabile*) to say that the duration of angel A or of angel B or C is nothing else but angel A or B or C … and nevertheless it appears to me that angel A was prior to angel B, not because it was in a prior time, but because it existed when B did not exist. And it can be said that that priority was temporal in a conditional sense, because if time coexisted with those angels, as is the case with those that now coexist with time, then angel A would have been prior in time to the succeeding time in which angel B or angel C existed … from an exterior denomination… even in a certain proportion….[60]

So, if Buridan's intellect is begging here, he is making suggestions of what it might be plausible to say, rather than asserting or demonstrating.

Begging or Borrowing?

Does the phrase "ideo quasi mendicare oportet intellectum humanum" in the context of God's absolute power help to elucidate what Buridan might recommend to the practitioner of any science who begins to doubt the principles of a given science for some reason

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[59] Ibid., 229-230. Buridan, *Physics*, f. 82rb, says: "adhuc est fortis dubitatio," "sequuntur alie magne inconvenientie" and "istud est difficile quia sensus vel imaginatio non cadit super talia sed solum intellectus et adhuc apud intellectum forte non potest convinci per rationes et ex sensibus deductas quod dicti casus sint possibiles."

[60] Buridan, *Physics*, f. 82ra: Sylla, "*Ideo quasi mendicare*," 230.

other than God's absolute power? In Buridan's Question 25 of *QuestPostAn*, discussed above, he said that the principles of geometry are not in fact certain, but the geometer assumes them as true and does not debate them. If there were to be any debate about geometrical principles, according to Buridan, it would be the task of the physicist or natural philosopher to engage in it, but not of the geometer.[61] In question 23 of *QuestPostAn*, after saying that natural science supposes principles from mathematics, Buridan goes on immediately to bring up the converse case in which it might be supposed that natural philosophy "descends" into geometry:

> Similarly, and conversely, the mathematician supposes [*supponit*] from natural [philosophers], as that the continuum is always divisible into divisibles, or also that it is not composed of indivisibles. For it is necessary to suppose this, because if a continuum were composed of indivisibles, almost all the demonstrations and conclusions of geometry would be false.[62]

Might "supposing" in this case be the result of the intellect so-to-speak begging? Could it be said that the mathematician "borrows" the principle that the continuum is always divisible into divisibles from natural science?

William of Ockham himself uses forms of the word *mendicare* in his *Dialogus*. Ockham's question is whether theology or canon law is the discipline most appropriate for determining heresies. His answer is that theology is the most appropriate discipline and that,

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61) See text in note 51. Buridan says the same thing in his *Physics* I, 5, f. 7ra, "Hec ergo declaro, quia magnum dubitabile est et fuit apud antiquos, utrum corpus esset compositum ex punctis indivisibilibus vel non, sed esset divisibile in semper divisibilia. Et illam dubitationem non potest geometer tractare per suam scientiam, sed tractanda est per phisicam vel per metaphisicam, et tamen geometer habet supponere quod continuum non sit compositum ex indivisibilibus, quia si esset compositum ex indivisibilibus, omnes pene conclusiones geometrie essent false."

62) Buridan, *QuestPostAn*: "Similiter, e converso, mathematicus aliqua supponit a naturalibus, ut quod continuum sit divisibile in semper divisibilia, vel etiam quod non sit compositum ex indivisibilibus. Hoc enim oportet quod supponat, quoniam si compositum esset continuum ex indivisibilibus, quasi omnes demonstrationes et conclusiones mathematicae essent falsae. Similiter, omnes scientiae supponunt a metaphysica et a logica universalissima quae sibi contrahunt…."

if canon law pronounces on heresies, it is because it "borrows" principles from theology:

> Therefore such a decision is known to pertain chiefly to theologians; it does not pertain to canonists, however, except in so far as they are known to borrow (*mendicare*) some theological matters from theologians.[63]

Here John Kilcullen and John Scott, who posted the text and translation of the *Dialogus* electronically have translated *mendicare* by "borrow"—does it make a difference if *mendicare* means "borrow" rather than "beg"? "Borrow" does seem to be a reasonable translation, since another text says that the moon borrows (*mendicat*) light from the sun.[64] The twelfth-century grammatical text *Promisimus* writes that the arguer sometimes borrows propositions in the sense of taking them fully formed.[65]

So here we have the suggestion that the geometer would take the principle that the continuum is always divisible into divisibles from natural science not as demonstrated (natural philosophy cannot demonstrate a principle of geometry, because that would cross disciplinary boundaries), but simply as a loan, to be used as an indemonstrable first principle (perhaps in the form of an implicit inference warrant). In this sense, a subalternating discipline does not prove the principles of a subalternate discipline, but may help

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63) <http://www.britac.ac.uk/pubS/dialogus/t1d1.html>, accessed 28 October 2008, bk. I, ch. 2, "Ergo ad theologos talis diffinitio principaliter noscitur pertinere, ad canonistas autem non spectat nisi in quantum aliqua theologica noscuntur a theologis mendicare." I discovered this and the following uses of *mendicare* by Googling *mendicare* with various other relevant words.

64) Alain de Lille, *Distinctiones dictionum*, as quoted by Virginie Minet-Mahy, "Quelques traces d'une 'théorie du texte' dans l'allégorèse en moyen Français…," *Le Moyen Age* at http://www.cairn.info/revue-le-moyen-age-2004-3-page-595.htm, accessed 30 October 2008.

65) Karin Margareta Fredborg, "The 'Promisimus,'" in *Medieval Analyses in Language and Cognition*, eds. Sten Ebbeson and Russell L Friedman (Copenhagen, 1999), 191-205 at 201 (found by Google search 28 October 2008): "Quid<am> dicunt quod disputator disponit argumentationem in animo suo antequam eam proferat, et sic in uno intellectu illa argumentatio concipitur. Quod falsum est, quia argumentator quando<que> mendicat propositiones."

to argue dialectically in their favor, perhaps in part by arguing against alternatives. Thus if the principles of a demonstrative science are called into question, another discipline such metaphysics may be called into action to deal with the problem, but that higher discipline will not demonstrate the challenged principles. Buridan, in the passages in question speaks of natural philosophy "treating" (*tractare*, *tractanda*), "explaining" (*indigent explanatione per superiores scientias*), and "expositing" (*indigent expositione*) the doubt whether the continuum is composed of indivisibles.[66]

Aristotle himself had suggested in the *Topics* that it was dialectic that had to deal with problems concerning scientific principles of established disciplines:

> We must say for how many and for what purposes the treatise [the *Topics*] is useful. They are three—intellectual training, casual encounters, and the philosophical sciences…. For the study of the philosophical sciences it is useful, because the ability to puzzle on both sides of a subject will make us detect more easily the truth and error about the several points that arise. It has a further use in relation to the principles used in the several sciences. For it is impossible to discuss them at all from the principles proper to the particular science in hand, seeing that the principles are primitive in relation to everything else: it is through reputable opinions about them that these have to be discussed, and this task belongs properly, or most appropriately, to dialectic; for dialectic is a process of criticism wherein lies the path to the principles of all inquiries.[67]

What Buridan and Albert of Saxony drew out of the *Posterior Analytics* was arguably an accurate representation of Aristotle's position. In the *Posterior Analytics* Aristotle describes the ideal structure of a

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66) Interestingly, in his *De continuo* Thomas Bradwardine attacks the problem of the composition of the continuum from every discipline before raising the issue whether he has committed a *petitio principii.* See John Murdoch, "Thomas Bradwardine: His Mathematics and Continuity in the Fourteenth Century," in *Mathematics and its Applications to Science and Natural Philosophy in the Middle Ages*, eds. Edward Grant and John E. Murdoch (Cambridge, 1987), 103-137; Edith Sylla, "Thomas Bradwardine's *De Continuo* and the Structure of Fourteenth-Century Learning," in *Texts and Contexts in Ancient and Medieval Science*, eds. Edith Sylla and Michael McVaugh (Leiden, 1997), 148-186.

67) Aristotle, *Topics*, trans. W.A. Pickard-Cambridge in *The Complete Works of Aristotle,* ed. Jonathan Barnes, vol. I (Princeton, 1984), Book I, ch. 2, 101a25-b4.

scientific discipline. He also describes how one might go about creating such a discipline. It has often been remarked that Aristotle's own scientific writings do not seem to conform to his ideal model as presented in the *Posterior Analytics*. In a typical natural philosophical work, Aristotle begins by examining previous theories on a given topic and then uses a critique of these theories to argue for what he considers the most reasonable position. In light of the passage from the *Topics* just quoted, it becomes clear that Aristotle's actual scientific practice follows the model he set out in the *Topics* for scrutinizing the principles of an already established science. Commentators on the opening of Book I of the *Physics* (where Aristotle argues that the natural philosopher need not argue against those who deny that all or some things move because it is a first principle of natural philosophy) reacted in various ways to Aristotle's suggestion that, if not the physicist, then the practitioner of some higher discipline might be called upon to debate the principles of physics.[68] In the special case of a subalternate science whose principles had become open to doubt, there was the suggestion that the subalternating science might prove (*probare*) the principles of the subordinate science.[69] Walter Burley, in his *Questions on Aristotle's Posterior Analytics* seems to countenance this possibility.[70] Others, including someone Burley called the "new expositor," and William of Ockham said that the proper principles of the subalternate science were only known by experience. Otherwise, all that a higher discipline can do is to argue, not demonstrate:

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68) Sylla, "The A Posteriori Foundations of Natural Science."

69) Based on Markowski, "Dialectische und Rhetorische Argumentation," 582, perhaps more should be made of the use of the word "*probare*": "Die *probatio* als eine der Begründungsformen kommt schon bei Johannes Duns Scotus und Wilhelm Ockham vor; eine intensive Beschäftigung mit ihr erfolgte erst in der Buridanischen Schule; seit dieser Zeit nahm die *probatio* einen zentralen Platz in dem Zweig der Logik ein, der Allgemeine Dialektik gennant wurde. Gleichzeitig mit dem Buridanismus verbreitete sich auch die *probatio* in Mitteleuropa und war auch an der erneuerten Krakauer Universität populär."

70) Sylla, "Status of Astronomy," 274 and n. 31.

> Second it should be noted that, against someone who denies the principles of a subalternate science, the subalternating science can argue; similarly both against someone who denies the principles of the subalternated science and against one who denies the subalternating science, both the metaphysician and the dialectician can argue. And thus he [Aristotle] says that it is not for geometry to argue against someone who denies geometrical principles, but for another science, that is a subalternating science, if it has a subalternating science, or for what is common to all sciences, that is metaphysics and dialectics. However, it should be known that first principles cannot be made evidently known by any common science, because the first principles of any science either are known per se (*per se nota*), and consequently cannot be demonstrated nor made known with an evident knowing (*notitia*) by others. Or they are not known per se, and then they cannot be made evidently known except by experience, which is not gained from any common science whatever. And thus no first principle of any subalternating science can be demonstrated. But against those denying it, it can be argued by the principles of a common science which they concede…. Similarly what they say is false, namely that all science can be resolved into principles known in themselves, because not all first principles in the special sciences are known in themselves, but some are known only by experience, without which they cannot be evidently known…. And thus that there is motion is not known except by experience, which nevertheless is assumed in natural science, as experience should be presupposed in natural science. Whoever wants to learn natural science without experience, strives after the impossible, because it is impossible to acquire natural science without experience, although it is possible to acquire natural faith without experience by believing what other people say.[71]

Perhaps, then, a medieval scholastic, faced with anomalies involving the principles of a demonstrative science, would turn to the method of dialectic to untangle the difficulties. Certainly as far as convincing others that a theoretical novelty was justified, the use of topical arguments might well be the best one could do. Still, it was the practitioner of the subalternate discipline who would take the initiative to suppose or borrow principles as undemonstrated starting points for his own work. The other alternative would be to gather new data from experience to establish new principles *a posteriori* as Ibn al-Haytham recommended doing in optics.

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71) Ibid., 274, n. 30; William of Ockham, *Expositio in Libros Physicorum Aristotelis,* in *Opera Philosophica*, vol. IV, eds. Vladimir Richter and Gerhard Liebold (St. Bonaventure, 1985), 44-46.

Buridan and Astronomy

Buridan himself did not claim to be either a geometer or an astronomer.[72] He declared himself (presumably as a natural philosopher) incompetent to resolve astronomical problems such as the problem that eccentrics and epicycles reproduce a given motion equally well.[73]

We know that astronomers not only reasoned mathematically about moving spheres or circles, but that they also took evidence from experience. It was astronomers who had collected the evidence of the motions of the stars and planets that they later used to determine their principles *a posteriori*, including the principle that the heavenly bodies move in uniform circular motion. Buridan knew that astronomers in antiquity had held theories different from those of modern astronomers.[74] There was a difference of theory, for instance, concerning the precession of the equinoxes. Depending on the observation of precession, some had added an extra celestial sphere or extra motions to the same sphere. Aristotle, Buridan said, did not

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72) For Buridan and mathematics, see J.M.M.H. Thijssen, "Buridan on Mathematics," *Vivarium* 23 (1985), 55-78.

73) John Buridan, *Quaestiones super libris quatuor De caelo et mundo*, ed. Ernest Moody (Cambridge, Mass., 1942), bk. II, q. 18, 211: "Utrum astra moveantur per se vel ad motum suae sphaerae…. Postea etiam mihi videtur quod difficile est demonstrare quod luna non habeat epiciclum; et difficile est etiam demonstrare quod luna habeat epiciclum; quia apparentia quae de luna salvantur per epicyclum et eccentricum, possunt salvari sine epiciclo per plures eccentricos quorum unus deferret alterum. Et hoc est considerandum in astrologia de motibus. Neutrum igitur est mihi demonstrabile, saltem per me; ideo etiam non est mihi demonstrabile utrum luna tali motu circumgirationis moveatur." Buridan makes a similar comment in *In Metaphysicen*, bk. 12, q. 11, f. 74ra: "Et ita etiam qui vellet ponere ecentricos sine epiciclis oportet ponere duos ecentricos, unum in altero, sicut ponimus ecentricum in concentrico. Et tunc salvaretur totum per illos duos eccentricos sicut salvabatur per ecentricum et epiciclum. Et breviter loquendo non apparet michi quod aliqua istarum viarum sit bene demonstrabilis nec etiam bene reprobabilis."

74) Buridan, *In Metaphysicen*, bk. XII, q. 9, f. 72ra: "Ista questio est valde difficilis et aliter esset respondendum secundum Aristotelem et Commentatorem, et aliter secundum fidei veritatem. Et adhuc etiam esset aliter respondendum tenendo principia Aristotelis secundum antiquos astrologos qui erant tempore eius et aliter secundum modernos."

believe that the eighth sphere moved with two motions, as other astronomers did. But to track these motions it had been necessary to observe celestial positions and to preserve the written records of these observations for as much as five hundred or a thousand years. During that time the records might have been falsified.[75]

In his questions on the *Physics* IV, q. 14, in the course of answering whether time is the measure of any motion, Buridan embarked on an extended disquisition about measurement and its problems, in the course of which he said that we cannot measure any motion precisely or to a point, because differences between two magnitudes may be too small to perceive.[76] Not long after Buridan, Nicole Oresme made a strong case for the probable incommensurability between the periods of the various planets, meaning that exact ratios would never be known.

As a natural philosopher, Buridan had asked what the number of intelligences is—whether, for instance, it is more than, equal to, or less than the number of celestial spheres. Is there one intelligence for every sphere, so that there would be intelligences for epicycles as well as the main orbs? Or might there be just one intelligence for the daily components of the motions of all the planets as well as the stars? It has been noticed, in this context, that from the viewpoint of theology Buridan suggested that the heavenly spheres might

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75) Ibid., bk. XII, q. IX, f. 72vb: "Unde apparet quod Aristoteles non credidit octavam sphera<m> moveri duplici motu, nec forte illud est bene certificatum, quia non potest certificari bene nisi per observationes seu reservationes scriptorum a longissimis temporibus, ut a quingentis aut mille annis. Et possibile est talia scripta fuisse falsificata. Immo aliqui secundum tales reservationes credidissent quod illa octava sphera moveretur contra motum diurnum in centum annis uno gradu. Visum est postea aliis quod post transitum quinque vel sex graduum ipsa revertebat. Et ideo talia non sunt bene certificata. Et ideo adhuc si illi antiqui vidissent illum secundum motum octave sphere ipsi posuissent preter spheram in qua fixe sunt stelle aliam ferentem ipsam vel revolventem sicut fecerunt de planetis."

76) Buridan, *Questiones super libros Physicorum*, f. 81ra: "Et notandum est etiam quod non possumus motus naturales omnino precise et punctualiter mensurare, scilicet secundum modum mathematice considerationis. Non enim possumus per stateram scire si precise libra cere sit libre plumbi equalis. Potest enim esse excessus in ita parva quantitate quod non perciperemus excessum sed sufficit sepe mensuratio ad prope iuxta illud quod de modico non est curandum."

be moved by impetus put into them by God at the time of their creation.[77] But, also from a theological point of view, he said that God might move all the spheres, and indeed all stones, by himself, without the aid of any intelligences. And, of course, he said, there are many legions of angels, not just the movers of the celestial spheres.

If it was appropriate to Buridan as a natural philosopher to ask about the number of celestial intelligences moving the heavenly spheres, it was not necessary for astronomers to deal with this question, because they only needed to know where the heavenly bodies were, the kinematics so-to-speak, in order to make astrological predictions, which was their main goal. In Buridan's words:

> And therefore it suffices for them to accept the easier imagination, according to which (if it were true) the celestial bodies would move with as many motions and with such velocities as they now move. And they do not have to care whether it is so in reality as they imagine. Now if the outermost sphere were posited to drag with it all the others and that sphere were posited to carry the epicycle, the same would result as far as concerns the various aspects of stars with respect to each other and with respect to us as now happens according to the position that we put forth. And therefore it is licit for them to posit another imagination, nor is it against their science, nor against the truth that they intend.[78]

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77) Buridan, *In Metaphysicen*, f. 734a. He introduces this idea as "una imaginatio nescio an fatua."

78) Ibid., f. 72va, "dico sicut dicit Commentator quod astrologi non habent curare a quibus corporibus corpora celestia moveantur, scilicet an a seipsis an ab intelligentiis: nec utrum ab uno motore an a pluribus: nec etiam utrum una sphera moveat aliam an non, sed sufficit eis scire quod tot motibus moventur et secundum tales velocitates quia solum per hoc volunt scire habitudines situales astrorum adinvicem et ad nos. Et ideo sufficit eis accipere faciliorem imaginationem secundum quam (si esset vera) corpora celestia moverentur tot motibus et talibus velocitatibus sicut nunc moventur et non debent curare utrum sit ita in re sicut imaginantur. Modo si ultima sphera poneretur secum trahere omnes alias et quod sphera poneretur ferre epiciclum, idem omnino proveniret quantum ad diversos aspectus stellarum adinvicem et ad nos sicut nunc provenit ex positione quam posuimus. Et ideo ipsi licite possunt ponere aliam imaginationem, nec illud est contra illorum scientiam nec contra veritatem quam intendunt. Sed de talibus imaginationibus eorum et aliorum philosophus habet inquirere que sit vera et que non."

For Buridan, attempting to speculate about astrophysics, the Ptolemaic system was not at all easy to supplement with physical causes—even if they were intelligences—of the observed motions. It does not appear that creating an astrophysics for a heliocentric system would have been hugely more difficult. In any case, Buridan the natural philosopher, observing mathematical astronomy from outside, thought that astronomers had every right to propose new imaginations—which is the same as new astronomical principles—when they thought it appropriate. But concerning their imaginations and those of others, the philosopher would have to inquire which were true and which not. Perhaps by this Buridan meant that it was the task of the astronomer to know that (*quia*) the planets move with certain velocities are are found in certain places, whereas it was the task of the natural philosopher to explain the cause, *propter quid*, of their motions.

The *Posterior Analytics* and Astronomy

In 1159, in his *Metalogicon,* John of Salisbury wrote:

> The science of the *Posterior Analytics* is extremely subtle, and one with which but few mentalities can make much headway. This fact is evidently due to several reasons. In the first place, the work discusses the art of demonstration, which is the most demanding of all forms of reasoning. Secondly, the aforesaid art has, by now, practically fallen into disuse. At present demonstration is employed by practically no one except mathematicians, and even among the latter has come to be almost exclusively reserved to the geometricians. The study of geometry is, however, not well known among us, although this science is perhaps in greater use in the region of Iberia and in the confines of Africa. For the peoples of Iberia and Africa employ geometry more than do any others; they use it as a tool in astronomy. The like is true of the Egyptians, as well as some of the peoples of Arabia.[79]

In this paper I have tried to examine the theories of science that appear in the questions on the *Posterior Analytics* of John Buridan,

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[79] John of Salisbury, *The Metalogicon* , V.vi, trans. Daniel McGarry (Berkeley, 1962), 212. Cf. Jens Høyrup, *In Measure, Number, and Weight. Studies in Mathematics and Culture* (Albany, 1994), 116-117.

with glances at Buridan's other works, and at the *Questiones subtilissime* on the *Posterior Analytics* of Albert of Saxony. I suggest that Buridan fine-tuned his theory of science as he attempted to deal with what he knew of the relatively successful scientific discipline of mathematical astronomy. Perhaps Buridan learned something, as John of Salisbury seems to propose, by looking at astronomy as a model of what a proper scientific discipline should be. In this case, the picture of separate scientific disciplines developed by William of Ockham, John Buridan, Albert of Saxony, and the *moderni* on the basis of Aristotle's *Posterior Analytics* and the actual practice of mathematical astronomers might have reinforced each other. On this model, astronomers might be willing to "recommence the inquiry into the principles and premises, beginning our investigation with an inspection of the things that exist and a survey of the conditions of visible objects," in Ibn al-Haytham's terms. In Buridan's view it was licit for them to do so without going against their science or the truth that they intended.